followed by instructions for the preparation and storage of oxygen, which is now so commonly employed in conjunction with hydrogen, or ordinary gas, for illuminating purposes. The preparation of slides of every description, micro-photography, and the process of making lantern enlargements, are all fully considered. One chapter is also devoted to the description of a few simple scientific experiments, which can be easily performed whilst projected on the screen. Finally, a few valuable hints are given to aspiring lecturers or entertainers who wish to avail themselves of the powers of the lantern.

The necessary references to firms which supply particular appliances have been made without any partiality. The addresses of such firms might have been given with advantage.

The book is full of practical hints from beginning to end. It is very readable, and we can confidently recommend it to all who are concerned with lantern matters in any shape or form.

Chemical Problems. By J. P. Grabfield, Ph.D., and P. S. Burns, B.S. (Boston: D. C. Heath and Co., 1888).

THE systematic part of this book occupies the first forty-six pages, the remaining forty pages containing reprints of examination papers. The first part contains general information as to chemical calculations and such matters, with some problems worked out which are likely to be of service to the elementary student; but there are a few points that appear open to improvement. The word reaction is used in its ordinary sense, and also to indicate an equation without the figures that indicate the numbers of the several molecules; to adopt the words of the authors, an equation is a balanced reaction. This appears to be a needless perversion of the meaning of a useful word. The student is told to "balance the reaction" "by repeated trials" of numbers, a method that is certainly very common, but entirely unscientific and unnecessary—in short, a method of cramming, and not a method of teaching. At p. 12 we read, without qualification, "that the weights of all gases are to each other as their molecular weights": it would be very inconvenient to the commercial maker of gases if the weights of his productions were so restricted. At p. 5 the word weight is used in yet another sense: "If we divide the *weight* of any element in the molecule, multiplied by 100, by the per cent. of that element, we will have the molecular weight." This looseness of language would, we fear, be confusing to most students and to many teachers. The volume will be chiefly useful to those who are preparing for the examinations indicated in the second part.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Recent Eruption at Vulcano.

IN a letter recently received here from Mr. Narlian at Lipari, that gentleman mentions some interesting facts relating to the recent eruption of Vulcano. Amongst others, he says that some weeks since some fishermen crossing between Sicily and Vulcano "found themselves in a boiling sea, the water bubbling up," "pieces of pumice coming up through the water." It appears also that the cable between Capo Milazzo and Lipari has been broken at about the same place, which is marked by "large rocks." He then says he has been unable to examine any of the pieces of pumice said to have come from the bottom of the sea.

Whether we have evidence here of a submarine eruption or of a fumarole it is difficult to decide, but further information we must hope will be forthcoming. It is possible that it is simply a submarine fumarole or spring, similar to the Sorgente Le Caldaje, which is met with in the sea at a point forming a triangle with the islets of Lisca Bianca and Bottaro between Lipari and Stromboli.

The following is the latest information in regard to the eruption of Vulcano (the letter is dated Lipari, December 12):—

"Some weeks since, the crater itself showed signs of diminishing activity. The eruptions are often at intervals of hours from each other, and never of such violence. Some days ago, a gentleman, Mr. Salino, says he had gone to the top of the mountain and seen the interior. This gentleman assured me that two-thirds of the old crater is filled up (he had visited the place eighteen years ago!); there was an opening of about 100 metres in diameter in the north-east side of the crater. No lava was seen."

From this it appears that the eruption is diminishing; that probably this filling up of the crater is due to the construction of a cone of eruption, or that it is being choked by the materials ejected from the new opening in the north-east side. Of course without examining the locality one can only conjecture what has really taken place. H. J. JOHNSTON-LAVIS.

Naples, December 15.

Natural Selection and the Origin of Species.

FOR the third time Mr. Thiselton Dyer announces that his judgment is opposed to the theory of physiological selection. But this is not the point that I am debating. I am not discussing the merits of my theory, or endeavouring to influence the opinion of a critic who, after having shown that he had not read my answer to the criticisms which he triumphantly reproduced as never having been answered, now tells us that he has "devoted a good deal of time to the study of" my "views." From the first I have restricted myself to meeting his specific allegations. Still restricting myself to the same ground, I find that there are but two points in his last letter which it is necessary for me to consider.

First, touching the inutility of some, as distinguished from the alleged utility of all, specific characters, Mr. Thiselton Dyer expresses impatience with me for putting what he regards as a "strained interpretation" on Mr. Darwin's writings.[1] Now, of all things in the world, this is exactly what I should most wish to avoid. But, rightly or wrongly, I am profoundly convinced that such strained interpretation as there is, here lies on the other side. Over and over again—and more and more emphatically the later the editions of his works—Mr. Darwin insists that he does not regard natural selection as the only agent which has been concerned in the origination of species, and therefore concludes—to quote only one additional passage from among many to the same effect: "No doubt the definite action of changed conditions, and the various causes of modification, lately specified, have all produced an effect, probably a great effect, independently of any advantage thus gained" ("Origin," p. 160). Moreover, towards the close of the last edition, he complains most bitterly of "steady misrepresentation" with regard to this very point (p. 421)—a complaint all the more forcible from its presenting the only note of bitterness that is to be met with through the whole range of his writings. Now, since his death, this "steady misrepresentation" has continued, until the post-Darwinian school have come to designate by the term "pure Darwinism" the very doctrine which he here so vehemently repudiates.

This doctrine of utility as universal was very clearly enunciated in his own life-time by Mr. Wallace, as "a necessary deduction from the theory of natural selection" ("Contributions," &c., p. 47), but, as just remarked, he expressly renounced it in his section headed "Utilitarian Doctrine, how far true." Therefore I say that, quite apart from all questions of biological theory, or merely as a matter of historical fact, any charge of "strained interpretation" must here lie at the door of those who seek to attribute to Mr. Darwin the opinions which have always been held by Mr. Wallace, and which have now been adopted by the school of Prof Weismann. Moreover, not only is there no sense or reason in speaking of the passages in question as "admissions wrung from a hostile witness," or as due to Mr. Darwin "admitting the possibility of explanations to which he really, however, did not attach much importance;" but by thus endeavouring to belittle his judgment on these points, the post-Darwinians are merely showing the weakness of their own. These passages are due to Mr. Darwin having clearly perceived that the doctrine of Mr. Wallace was neither sound in logic nor true in fact. Not sound in logic, because it does not follow as a "necessary deduction from the theory of natural selection," that all specific characters must be adaptive—it being sufficient for the theory if only some such characters are adaptive in each case, as Mr. Huxley has recently shown; not true in fact, because any number of cases (such as those given by Mr. Mivart) can be quoted to the contrary. Therefore, as I said two years ago in the *Nineteenth Century*, those who seek to encumber the theory with this illogical deduction are merely giving occasion to the enemy; it is a gratuitous dogma, standing like the feet of clay in a figure of iron.[1]

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[1] He says: "What, however, I view with less patience than his unsustained generalizations, is his persistent attempt to place them on the shoulders of the Darwinian theory." Elsewhere, however, the crown and front of his charge is that I have sought to shrivel up the Darwinian theory to very small dimensions—nay, that I have roundly denied it altogether, if my words have any meaning. How these contradictory criticisms are to be reconciled I must leave their author to explain.

But to pass on to the second point. In my last letter I challenged Mr. Dyer to justify his statements that I had roundly denied the agency of natural selection as "the mechanical means by which evolution has been brought about," and that the theory of physiological selection "shrivels up the part played by natural selection to very small dimensions." In answer, he quotes a passage from my paper, and agrees with me that what it says is substantially the same as what the *Times* said. But he still fails to see that this is totally different from what he himself said. In other words, unlike the *Times*, he does not perceive the validity of my distinction between natural selection as a theory of species and as a theory of adaptations. Physiological selection, he thinks, shrivels up natural selection, because, if a true principle in Nature, it must play an important part in the origination of species, and, in whatever measure it does this, it must in a corresponding measure detract from the importance of natural selection. Having at last got him to show that this is his way of regarding the matter, I must first of all repeat that natural selection, besides being a theory of the origin of species, is also something very much more: it is a theory of the cumulative development of adaptations, wherever these occur. In fact, it is, as I have before said, *primarily* a theory of adaptations in all cases, and only becomes *incidentally* a theory of species in those cases where the adaptations happen to be of merely specific value. It is now perfectly evident that Mr. Dyer fails to perceive this distinction; hence his misunderstanding of my views, and hence also the present correspondence. He regards the "origin of species" as synonymous with, and therefore as covering the whole field of, "organic evolution": therefore he accuses me of roundly denying natural selection as "the mechanical means by which that evolution has been brought about," on the ground that I have suggested a supplementary theory of the origin of species. Such being manifestly the impression under which he has read my paper, it is no wonder that in the process he has been, as he says, "completely befogged." I will now endeavour to clarify the matter by explaining at length what I had supposed the readers of my paper would have recognized for themselves.

It is quite true that the evolution of adaptations depends upon the evolution of species, the serial succession of which, in any given line of descent, is the necessary means (through the struggle for existence) to the gradual development of adaptations in that line. But it is of no consequence how many "indifferent" characters these successive species display, provided that they also display, in ever-improving degrees, the particular adaptive characters which are in course of evolution. A bird's wing, for example, is an adaptive structure which cannot be evolved as a merely specific character; it requires to be slowly built up through the lives of an enormous number of successive species, which ramify into genera, families, &c., as the process goes on. Now, throughout this process it is a matter of no consequence how many *other* features of a *non-adaptive* kind arise among all these innumerable species: it is enough, as regards the evolution of a wing, that at each stage of the process *some* of the species should present slight improvements on their predecessors in respect of this adaptive structure. Physiological selection, sexual selection, geographical isolation, "changed conditions" as to climate, &c., or any other "factor," may all the while have been originating any number of *species*, without reference to their wings, though, at the same time, natural selection was continuously promoting the development of wings in *genera*, *families*, and *orders*. In short, species are like leaves, successive and transient crops of which are necessary to the gradual building up of adaptations, while these, like the woody and permanent branches, grow continuously in importance and efficiency through all the tree of life. Now, it is the office of natural selection to see to the growth of these permanent branches: physiological selection has to do only with the deciduous leaves. Hence, although natural selection has likewise an immensely large share in the origination of species (*i.e.* has to do with all species which are distinguished by adaptive characters *peculiar to themselves*), this, in my view, is really much the least important part of its work. Not as discovering *an* agent in the differentiation of species, but as revealing *the* agent in the genesis of adaptations, do I regard Mr. Darwin's theory as the greatest generalization in the history of science. If this view of the matter betrays on my part, as Mr. Dyer says, a fundamental misunderstanding of that theory, I shall be greatly obliged to him for showing me wherein the misunderstanding consists. In the event of his doing so, I will cheerfully renounce the inquiry on which I am engaged, for then, no doubt, my theory would be found in opposition to—and not, as I suppose, in co-operation with—the theory of natural selection. On the other hand, should he fail to meet this request, I shall have "reluctantly to arrive at the conclusion" that the "fundamental misunderstanding" in this matter, like the "strained interpretation" previously considered, lies the other way.

It will now, I trust, be sufficiently evident why I differ *toto cœlo* from Mr. Dyer where he concludes that the theory of physiological selection shrivels up the theory of natural selection. In point of fact, the former theory stands to the latter in precisely the same relation as does the theory of sexual selection. In both these supplementary theories, it is the origin of species that is concerned, and so concerned with reference to characters that are non-adaptive. The cases being thus precisely parallel, I should like to know whether my present critic regards one of Mr. Darwin's own theories as shrivelling up the other.[1] Assuredly, Mr. Darwin himself did not think so, because he clearly perceived that the "origin of species" constituted but a small part of the whole field of "organic evolution." It is true that he entitled his work "The Origin of Species by means of Natural Selection," and therefore in my paper I was careful to point out that, "if it appeared somewhat presumptuous to have insinuated that Mr. Darwin's great work on the 'origin of species' has been mis-named," there were passages in the work itself which fully justified me in my definition of his theory. My critic now quotes this explanation as justifying his statement that I intended to deny the agency of natural selection altogether! I do not quite know how to meet an opponent who resorts to such strange devices; but I may at any rate assure him that in my opinion no more appropriate title could have been chosen by Mr. Darwin for his great work than the one which he did choose; and if I spoke of that work as having been mis-named, I thought I had made it clear enough that I was "strictly speaking," or speaking to a point of logical definition. Moreover, at the time when the work in question appeared, the problem as to the origin of species was, as its author says, "the mystery of mysteries." But now that we have all come to recognize species as nothing more than pronounced varieties, it appears to me a curiously interesting example of the "survival" of traditional modes of thought, that so many systematists still continue to regard the value of Mr. Darwin's theory to consist in what is really its least important function. The result has been clearly displayed in the present correspondence :—

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[1] In connection with this point I have to express regret for a verbal error which Mr. Dyer has already pointed out in my last letter. But, from what he says of Prof. Nägeli's essay, it appears to me that he cannot have read it. At any rate, it is in no way confined to "the inutility of family characters," and in considering it Mr. Darwin supplies more than two pages of instances to illustrate his argument therewith, ending with the general conclusion:—"We thus see that with plants many morphological changes may be attributed to the laws of growth, and the interaction of parts, independently of natural selection." To ignore all such passages, or to regard them as admissions," is assuredly—once more to quote my critic's words against himself—"entirely to misapprehend their significance, or the spirit in which they were made."

[1] In this connection it is interesting to note that Mr. Wallace has always been the principal opponent of the theory of sexual selection, as he now is of the theory of physiological selection. Moreover, the reason of his opposition in both cases is that he thinks such subordinate theories of the origin of species must fail to find a *locus standi* in the presence of the greater theory of natural selection: the latter, in his view, must necessarily "shrivel up" the former. Now, his arguments against sexual selection are incomparably stronger than those which he has advanced against physiological selection (compare "Tropical Nature," pp. 192–211, with the *Fortnightly Review* for September 1886, and the *Nineteenth Century* for January 1887); yet they failed to influence the judgment of Mr. Darwin, whose very last words to science—read a few hours before his death to a meeting of the Zoological Society—were: "I may perhaps be here permitted to say that, after having carefully weighed, to the best of my ability, the various arguments which have been advanced against the principle of sexual selection, I remain firmly convinced of its truth."

> There's such divinity doth hedge a species.
> That science can but peep to what it would.

London, December 8. GEORGE J. ROMANES.

Engineers *versus* "Professors and College Men."

PROF. TAIT in his recent letter says that the only meaning the expression $\frac{Mv^2}{2g}$ conveys to his mind is the product of a mass by a length. But how does he measure his mass and his length? Is the mass to be measured in pounds or kilogrammes, or to be measured in units of g pounds or g kilogrammes? And is the unit of length the foot, or the metre or centimetre? so that g is variously 32, 9·81, or 981.

These are the points which are slurred over by "Professors and College men," but are of fundamental importance to practical engineers, who dare not trust to a formula till they have verified it numerically.

Let me conclude by giving Prof. Tait a question, selected from College text-books :—

"What is the meaning of $\frac{Mv^2}{2g}$ when the unit of area is one-tenth of an acre, the numerical value of g is 2, and the unit of weight is the weight of unit volume of the standard substance (the substance of which the specific gravity is unity)?"

I think he would be amused by the variety of answers he would receive, although the answers might individually be all correct.

December 17. A. G. GREENHILL.

Mr. Dodgson on Parallels.

MR. DODGSON has written to me thanking me heartily for my "interesting and helpful review" of his "New Theory of Parallels." He admits his slip in the corollary on p. 11, and supposes, as I had myself thought, he took ADC to be the triangle required instead of ABF. "But there is *one* criticism of yours which, if true, would vitiate the whole treatise. May I ask you to reconsider the point, and, should you see reason so to do, to notify to the readers of NATURE that you withdraw it? You say that, in Props. viii., xi., I tacitly assume that the 'amounts' of triangles are either *all* greater than two right angles, or else *all* less. . . . Such an assumption would indeed be monstrous." I willingly accede to Mr. Dodgson's request, as the following form of his argument, supplied in his letter to me, does away with my difficulty. "Either (α) there is a triangle whose 'amount' = two right angles, or (β) there is none. If (β) be true, then either (β1) *all* triangles have *greater* 'amounts,' or (β2) *all* have *less* amounts, or (β3) some have *greater* amounts and all others *less*. Now (β1) is proved impossible, in Prop. viii. ; (β2) is proved impossible in Prop. xi. ; (β3) may easily be proved impossible, by means of Prop. vii. Hence (β) is impossible. Hence (α) is true." It will be well, if, in a future edition, the missing link of (β3) be supplied. One other point puzzles Mr. Dodgson. It is my remark on Prop. vi. : "How are the figures to be constructed if $n>2$?" Mr. Dodgson says : "It surely does not need pointing out that the operation of bisecting an angle may be repeated *ad libitum*." Certainly not. But what I meant was the effect of these bisections upon the resultant chords. The figures to the proposition are incorrectly drawn : in the one figure BD, DC, and in the other BE, ED, DF, FC are *not drawn* greater than the radius, and my point was not the bisections but the enlargement of the figure : thus if $n = 3$, we should have eight triangles, vertices at the centre A, with the sum of their angles greater than 480°. My apology for thus trespassing upon valuable space is my desire to meet Mr. Dodgson's natural wish, and by pointing out what I thought were faults in his "interesting" *brochure* to enable him to make it more perfect in after editions. R. TUCKER.

University College School.

The *Porcupine* Echinoidea.

THE researches lately published by the Drs. Sarasin upon the anatomy of the Echinothuridæ, render a careful reconsideration of the types of Asthenosoma collected by the late Sir Wyville Thomson, during the voyage of H.M.S *Porcupine*, absolutely necessary in my opinion. The species were described in the Philosophical Transactions for 1874. I shall be much obliged if information can be sent me regarding the whereabouts of the specimens which were figured by Wild, *i.e.* the type-specimens of *Calveria* (*Asthenosoma*) *hystrix*, Wy. Th. ; *C.* (*Asthenosoma*) *fenestrata*, Wy. Th. ; and *Phormosoma placenta*, Wy. Th.

P. MARTIN DUNCAN.

Angry Birds.

IN reference to the notice of a fierce pheasant mentioned by Mr. Maw in the number of NATURE for December 13, I would refer him to my "Observations in Natural History" (p. 172), in which I have spoken of a daring cock pheasant, which I saw myself, while walking in the grounds of a friend in Cambridgeshire. This bird was in the habit of attacking any persons that approached near the spot where he was. Some woodcutters at work on the grounds had to protect their legs with strong leather gaiters. L. BLOMEFIELD.

Bath, December 18.

TWO years ago, whilst walking across a fallow field here, I heard a fluttering of wings, and received a violent blow on the back of the neck from a partridge : before I could recover myself she struck the back of my head and knocked my hat off. Although I had a heavy stick, I could not drive off the bird, who made a loud noise, and now attacked me in front. As I walked rapidly off, the bird followed and struck at me many times, attacking my head and shoulders with the greatest determination and violence. W. G. SMITH.

Dunstable.

PRESENTATION OF A PORTRAIT OF PROFESSOR A. W. WILLIAMSON, F.R.S., TO UNIVERSITY COLLEGE.

ON Wednesday (the 12th inst.) a portrait of Prof. A.W. Williamson, late Professor of Chemistry to University College, London, was presented to the College by Sir Henry E. Roscoe, M.P., F.R.S., on behalf of the committee of subscribers. The portrait is painted in oil by the Hon. John Collier. The presentation took place in one of the lecture-rooms, the chair being taken by the President of the College (Mr. John Erichsen, F.R.S.) ; and amongst those present were Sir F. A. Abel, Prof. Bonney, Prof. H. Morley, Dr. J. H. Gladstone, Prof. George Carey Foster, Dr. Atkinson, Prof. Ramsay, Prof. Thorpe, Prof. Marks, Prof. Russell Reynolds, and other Professors, and a large number of the past and present students of the College.

Dr. W. J. Russell, on behalf of the Committee, for whom he had acted as Treasurer, said that judging from the subscription list there was a large number of the former colleagues of Prof. Williamson who had subscribed to this portrait ; and it would no doubt be very pleasant to him to know that members of all the Chemical Societies in England had liberally subscribed towards the portrait ; and further, that many of the subscribers had not satisfied themselves by sending formal contributions, but had written to him (Dr. Russell) expressing their great esteem and regard for Dr. Williamson. The subscriptions did not come only from various parts of Great Britain, but from France, Germany, Switzerland, Italy, Russia, and even so far afield as the United States, Jamaica, India, and Japan. He thought this was all that it was necessary for him to say in order to indicate the high value which the subscribers attached to the great scientific attainments and labours of Dr. Williamson, whose intimate friends and old pupils, those who knew him best, now came forward to pay him this mark of their esteem and regard.

Sir Henry Roscoe, M.P., said :—I consider it, sir, a privilege that, as an old pupil and an attached friend of Dr. Williamson, I should have been chosen, on this occasion, to present his portrait—which I think you will